[R. 6.] THE LAWS OF DYNAMICS, AND THEIR
TREATMENT IN TEXT-BOOKS.

AMONG the defects to be found in the treatment of the elements
of Dynamics in many current text-books, the most obvious is the
way in which the "laws of motion" are customarily introduced
without any clear explanation as to the base, or axes, relative to
which the motion is to be reckoned. Attention is very likely
called to the fact that any statement about the motion of a body
must, if it is to have any meaning, include the specification of a
base; but in the statement of the law of inertia we are apt to
find all this thrown to the winds, and the reader left to discover
for himself, as best he can, what base is meant to be used. If a
straightforward explanation can be given on this point, why is
such a mystery made of it? A reader, approaching the subject
for the first time, is apt to draw the inference that the choice of
a base will somehow or other turn out to be immaterial. State-
ments may be met with which give encouragement to this view.
Thus, in some of the text-books, the law of inertia is said to be
merely a definition of force (or of both force and time); and this
may be put in such a way that it is difficult to escape the con-
clusion that we are to recognise as forces simply mass-accelerations
relative to an arbitrarily chosen base; although the abandonment
of the law of action and reaction, which this would imply, is
not really intended. It is more common to find Newton's laws
of motion quoted, and illustrated by examples of motion relative
to the earth, the question of a base being left vague. It would
be a more intelligible course, and possibly better for some
purposes, to begin an elementary treatment of the subject with
an approximate theory of motion relative to the earth only. In
some books we find a complication produced by preliminary
remarks as to none but relative motions being knowable, followed
by expositions of the knowableness of *absolute* rotation and

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fixedness of direction, without either apology or explanation. In most of the text-books the point of view adopted is rather obscure. An exception must be made in favour of Love's *Theoretical Dynamics*, which has the merit of being intelligible; but the procedure employed seems to be unduly ponderous and complicated.

We owe to Galileo the discovery that a fruitful theory could be based upon uniform velocity and laws of acceleration. This theory was developed by Huyghens and others, and was completed by Newton. We still employ it in the form in which it left Newton's hands. In this theory, as Newton left it, the selection of a base, relative to which motion is to be reckoned, is a fundamental feature. The old practice was to call motion relative to such a base "absolute" or "true" motion, a thing to be distinguished at the outset from motion relative to any particular body. Anyone who will take the trouble to look through the literature of the subject, will see that this practice of postulating the conception of absolute motion, or something equivalent, as the first step in the statement of the theory, was on the whole steadily maintained up to the beginning of the present century; but that, since that time, this point has been generally slurred over, or treated less conspicuously. The habit of mentioning it having once been discontinued, the writers of elementary text-books seem to have been almost content to leave it alone; and the treatment of it has been mainly confined to the comparatively few books in which a critical discussion of the foundations of dynamics has been attempted. Some explanation of this fact can be found in the very reasonable discrediting of the term "absolute motion," and the difficulty of finding an adequate substitute for it; but it must also be attributed, to some extent, to downright illogical treatment of the subject, and a slipshod acquiescence by some writers in current forms of statement whatever they happen to be. Even modern critical writers are not wholly in agreement with one another, at any rate in form of statement. Accordingly there is some confusion; and we may fairly demand, for those who merely wish to understand the theory, a more straightforward way of approaching the point in question than they commonly meet with at present.

So far as we can judge from the *Principia*, it seems that Newton felt no difficulty about postulating an absolute space, in which bodies occupy absolute positions. True, or absolute, motion is the translation of a body from one absolute position to another, and is to be distinguished from apparent or relative motion, that is to say motion relative to another body. All that this practically amounts to is that a base is postulated relative to which motion is to be reckoned for the purpose of the theory. But the postulating of a base of reference will not be of any use

if it cannot be defined, and we are warned that it is not to be defined in terms of coordinates measured from any known body. Newton accordingly goes on to explain how the base is to be defined. This he does very fully, calling attention in particular to the phenomena attending rotation relative to the base in question. He appears to believe in the reality, so to speak, of one definite base, such as might be furnished by an ether; but he indulges in no speculations, and, beyond referring to it as a thing to be sought, pretends to no knowledge that he is not entitled to. The following sentence from the *Principia* will serve to indicate the general drift of his discussion of the subject: "It is indeed a matter of great difficulty to discover, and effectually to distinguish, the true motions of particular bodies from the apparent; because the parts of that immovable space in which those motions are performed do by no means come under the observation of our senses. Yet the thing is not altogether desperate; for we have some arguments to guide us, partly from the apparent motions, which are the differences of the true motions; partly from forces, which are the causes and effects of the true motions." In fact our base is to be *defined* by the fact that all accelerations relative to it correspond to forces. So, if we are able to detect and classify forces, we have the means at our disposal for discovering a base such as the theory requires; assuming that it exists, that is to say that the theory is true. If our knowledge of the motions of bodies in the universe is limited, any base which is established must be regarded as so far provisional; and we see that it has one essential peculiarity, namely that, whatever base is specified for the purposes of the theory, any other which moves relative to it uniformly and without rotation will do equally well. But how are we to detect forces, and what do we mean by force? This, Newton says, is set forth more at large in his book. He at once proceeds to deal with the question; but the whole of the *Principia* has a bearing on it. In fact, in the classification of forces, the law of gravitation occupies so important a place that no one would choose to deal fully with the question without taking this into account. Let us, however, consider, as a matter of logic, what means we have of detecting "forces." The key to the situation is to be found in Newton's third law of motion. The property of forces by means of which they are to be traced is their mutual character. This mutual character constitutes causal relations between the motions of portions of matter. These can be investigated by the methods of physics, and can, as a matter of fact, be rather concisely classified, with regard to the conditions under which they occur, a few types (pressure, gravitation, etc.) comprehending all that are known. The possibility of this classification is a fundamental feature of the subject.

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There are certain cases in which a physical symmetry of the conditions attending a relation between the motions of bodies can be approximately secured; though, of course, complete symmetry of environment is unattainable. One such case is that of the pressure between like surfaces of similar bodies in contact; another is that of the repulsion between like bodies, symmetrically presented to one another, and similarly electrified. These are cases in which we can experiment with the conditions and isolate their effects, because we can remove them and reapply them, and so satisfy ourselves that, as we might expect, the corresponding motion relations have a certain symmetry. If the bodies are particles, such symmetrical conditions correspond to equal and opposite accelerations of the bodies, in the line joining them, relative to any base formed by a body which is found, by the test of successive applications and removals, to be independent of the experiment. But we also find that, when the conditions are made unsymmetrical, the corresponding accelerations of like particles are still equal and opposite, relative to such a base; and that, when the particles are not alike, the ratio of their accelerations can be expressed by the introduction of mass. The theory is that we can choose a base of reference and a mass distribution (with certain characteristics, such as that of addition by superposition), so that the accelerations of all particles shall be entirely made up of components occurring in pairs which have the reciprocal character, with ratios expressible as ratios of masses.

The treatment of the subject at the beginning of the *Principia* is not very concise, and may even be called clumsy; but it compares very favourably with many of the modern expositions of the theory, and shews how sound a grasp Newton had of the essential points to be brought out in the statement. It is a strange thing that there is no modern English translation of it, or of any portion of it of sufficient length to give an intelligible view of the way in which the subject is dealt with.

During the eighteenth century, so-called "absolute motion" continued to occupy the conspicuous place in text-books to which its scientific importance entitled it; though it was a good deal confused by a tendency among writers of that time to indulge in futile *a priori* argument, which Newton did not do. Even in Euler's *Mechanica* (1736) this tendency will be found. The use of the word "absolute," however, not unnaturally provoked criticism in certain quarters. Berkeley expounded the obvious objections to the conception, which it might be supposed to imply, of something more than the facts warranted. Euler did something towards providing explanations to put the matter on a satisfactory footing. Maclaurin, who seems to have set himself to answer Berkeley, did not show much appreciation

of his point of view. The first draft of Maclaurin's *Account of Sir Isaac Newton's Philosophical Discoveries* was written on the occasion of Newton's death in 1727, and the book was published posthumously in 1748. It is interesting as shewing how the situation presented itself to an acute scientific writer of that time. With reference to objections, such as Berkeley's, he wrote as follows : "I know that some metaphysicians of great character condemn the notion of absolute space, and accuse mathematicians in this of realising too much in their ideas: but if those philosophers would give due attention to the phenomena of motion, they would see how illgrounded their complaint is. . . . . . . It were easy to enlarge on this subject, and to shew that there is no explaining the phenomena of nature without allowing a real distinction between true, or real, and apparent motion, and between absolute and relative space." The distinction between motion relative to any casual body and motion relative to the base which is postulated in the theory is fairly well set out, on the same lines as Newton's own account of it. The evidence afforded by the variations in the time of the oscillation of a pendulum in different latitudes is one of the examples given, as bearing on the earth's rotation. Thirty years later, in the notes to Thorp's translation of Book I. of the *Principia*, we find "the metaphysicians" still being gibbeted. Laplace began the *Mécanique Céleste* (1799) with the statement that a space, real or ideal, must be conceived relative to which the motion of bodies is to be reckoned. Then came the general tendency, in text-books, to drop all preliminary specific reference to a base as part of the theory.

The present practice is apt to make the subject puzzling to a beginner; the partial treatment of the question of the base, which he is likely to meet with, being as a rule not very lucid. Moreover, the metaphysicians have still a reasonable right to object to the form of such statements as that the *absolute* rotation of the earth can be *proved*, as it is by Foucault's pendulum and other such methods, and that the invariable plane of the solar system gives an approximately *fixed* direction. In fact, it seems a mistake to employ unnecessarily forms of statement which are only intelligible to experts, especially in connection with a subject of such general interest. In considering our present position in the matter, we may perhaps look forward with some confidence to a connection between the law of inertia and the ether; but, however this may be, the dynamics of ordinary matter must for the present be capable of standing on its own bottom; and such base of reference as the law of inertia involves, should be introduced as a thing belonging to the theory, independently of any wider view of physics as a whole. This is the position which it has practically held, and the chief thing

that has been lacking has been any general agreement in the matter of clear and precise statement. For the purpose of elementary treatment the introduction of a name, chosen so as to imply that the base is merely part of the machinery of the theory, would do something towards putting it on a satisfactory footing. Such a name as "Newtonian base" would serve this purpose; let us adopt this name in default of a better one. "Newtonian base" would then figure at the beginning of any book on dynamics, just as "absolute motion" does in Mrs. Marcet's *Conversations on Natural Philosophy*, where it must be admitted that Mrs. B. does not explain it to Caroline in a way that carries much conviction. The motion of a stone on the surface of smooth ice is a stock example of the first law of motion. In Galileo's dialogues Simplicius objects to horizontal motion on the earth being regarded as rectilinear, on the ground of the earth's curvature, and has to be told that the treatment is only approximate. A point of more importance, to which attention should at least be called, is whether the motion is reckoned relatively to a base which can be regarded as approximately Newtonian; and how this case differs, if at all, from, say, the case of a bead projected along a wire stretched horizontally across a compartment in a railway train. The fact that we have no knowledge of any but relative motions, whether they are rotations or of any other kind, may be insisted on as much as is thought convenient. These relative motions are what the theory proposes to disentangle.

The theory is enunciated for the whole universe, thus in its statement going, as all general theories do, beyond the range of verification. From this there is no reasonable escape; it is not convenient to try to impose any limit to its range in space so long as it serves our purpose. In the meantime we may bear in mind what the actual evidence amounts to. The word "provisional" is a convenient one to prefix when we wish to call attention to the fact that a certain base, employed for a given system, can only be regarded as a Newtonian base if the existence of bodies outside that system is ignored; but as any actual base is provisional the prefix may often be dropped. The use of a provisional base should, according to the theory, involve us in a certain amount of error, except in the case of the ignored accelerations, relative to a true Newtonian base, being the same for all particles of the limited system under consideration; but such errors may happen to be small enough to be negligible for some or all purposes. We may often confine our attention to only a limited portion of a system, taking into account, as "external forces," the forces between this portion and bodies outside it; but here a base must be adopted which is Newtonian for the whole system to such degree of approximation

as the case demands. There is of course, however, nothing to prevent us from using the methods of kinematics to effect a transformation to any other base which we think it convenient to use. Such a transformation (either complete or approximate, according to the needs of the case) may, if we please, be put into the form of an application of suitable so-called external forces, regardless of the fact that they do not correspond to any actual bodies outside our system.

The provisional Newtonian base which may be employed for the solar system, bodies outside that system being ignored, appears to be a very good one. No feature in the relative motions of the bodies composing the solar system has been traced to the influence of the stars. A calculation of the order of magnitude of the gravitational perturbation of Neptune, due to a Centauri, on the basis of current estimates of the parallax and mass of the star, will show that this, assuming it to exist, is insignificant in comparison with quantities at present measurable. This is the most favourable case that we know for the appearance of such a perturbation. Moreover, no rotation of this base relative to the directions of the fixed stars has been detected—a fact which has some bearing on the question of motion relative to the ether (see Pearson's *Grammar of Science*, second edition, p. 289). For terrestrial motions the use of a provisional Newtonian base for the earth alone (rotating relatively to the earth) gives good results, the most notable exceptional case being that of the ocean tides. A base fixed to the earth, such as we commonly use for terrestrial motions, is not a very close approximation to being a Newtonian base, and an approximate correction for its diurnal rotation is always employed. The correction is introduced in the form of an external force, and we are apt to lose sight of it, because it is combined with gravitation to make up what we call weight. This is the vertical mass-acceleration which Galileo dealt with. The variation of it in different latitudes was not known by Galileo, and the fact that this variation is partly due to the rotation component seems to have been first suggested by Huyghens. There are many ways in which the errors due to the correction being only approximate may be exhibited; one of the most celebrated examples being that of the deviation of a falling body from the plumb line vertical, which was first worked out by Newton. Our knowledge of the relative motions of bodies outside the solar system, and of the solar system as a whole relative to them, is rather scanty, but is in no way inconsistent with the view that our theory may be applicable throughout the whole of the known universe.

The range of applicability of the theory has to be regarded from another quite different point of view. No dynamical theory can satisfy us which does not embrace molecular systems,

and finally the behaviour of the ether has to be coordinated with that of material bodies. The question of the modifications demanded for these purposes is too large a one to be entered upon here. It includes the questions whether the Galileo-Newton theory should not be combined from the beginning with the theory of energy, instead of being treated apart from it, and whether any theory in terms of force can be permanently maintained.

A concise statement of the theory may be arranged as follows. We have to conceive all matter as divided into particles, small enough for the motion of each, relative to an independent base, to be specified by a single velocity, and its position by a point. And we have to suppose each portion of matter to be furnished with a scalar quantity called its mass, which has no relation to its position, and is such that the mass of a body is the sum of the masses of its parts, and that the masses of two bodies alike in all other respects, except as to position, are equal. (The permanence of mass, under chemical and other changes, should be regarded as an independent experimental result.) The theory then consists of a single law, namely, that a base can be so chosen and masses so assigned that the mass-accelerations of all particles can be analysed into equal and opposite pairs.

To complete the nomenclature of the subject, we have to add that each member of each of these pairs is called a force, and that we find it convenient to apply the name force also to any component of a force or resultant of several forces. The detection of forces and their classification, and the dependence upon this of the discovery of a Newtonian base, has already been discussed; also the treatment of limited systems.

Mass ratios are simply the negative reciprocals of acceleration ratios relative to a Newtonian base; but the word mass is needed for the statement of the fact that acceleration ratios can be expresssed by means of a quantity attached to each particle. A fundamental experiment as to the expression of acceleration ratios by means of mass can theoretically be arranged as follows : Take three particles, A, B, C, and contrive in succession forces between them two at a time. Let p be the ratio of the magnitude of acceleration of A to that of B corresponding to the force between them, q the similar ratio of acceleration of B to that of C, and r the ratio of the acceleration of C to that of A ; then it will be found that pqr is equal to unity. For the purpose of this experiment a base fixed to any body which is independent of the experiment will serve theoretically, provided that we have a means of measuring acceleration without depending on the measurement of distances moved through. If we cannot perform such a measurement, or if we cannot be satisfied that a body independent of the experiment can be taken, a

Newtonian (or approximately Newtonian) base must be used. Practically the earth may be employed, without restriction, for experiments on a small enough scale. The equality or inequality of masses can be tested by collision experiments relative to any base as to which we can be satisfied that it has not a sudden change of velocity, relative to a Newtonian base, at the moment of impact. The identification of mass has been aided by the fact, originally tested by Newton, that weight, the character of which has already been referred to, is, at any specified place on the earth, proportional to mass; also by the recognition of a classification of substances with density as a quality expressing their relation to mass.

In order to show clearly what we are doing when we employ a base attached to a body which is "independent" of a certain experiment, it may be worth while to write down expressions for accelerations relative to a given base in terms of coordinates referred to a frame (as we will call it for the sake of distinction) which has a given motion relative to the base. Suppose the frame to carry rectangular axes of x, y, z, and let f_1, f_2, f_3 be the components in these directions of the acceleration of the origin relative to the base, which we will suppose to be Newtonian; and let p, q, r be the components of the angular velocity of the frame relative to the same base. Let x, y, z be the coordinates of a particle of mass m. Then the x component of the acceleration of the particle relative to the base is

$$f_1 + \ddot{x} + 2\dot{z}q - 2\dot{y}r - x(q^2 + r^2) - y(\dot{r} - pq) + z(\dot{q} + rp).$$

Let a, β, γ be the components of acceleration of the point of the frame at which the particle happens to be, and A, B, C the components of acceleration of the particle relative to a base kept parallel to the given one but attached to this point of the frame; then the above expression is equal to $a + A$, and we see that

$$a = f_1 - x(q^2 + r^2) - y(\dot{r} - pq) + z(\dot{q} + pr);$$
$$A = \ddot{x} + 2\dot{z}q - 2\dot{y}r.$$

The components of the resultant of forces acting on the particle are $m(a + A)$, $m(\beta + B)$, $m(\gamma + C)$. Now make an experiment consisting of the imposition of a new force X, Y, Z on the particle, without the motion of the frame relative to the base being thereby affected; the frame thus being independent of this experiment. The only terms in the above expressions which we alter at the moment of imposition are $\ddot{x}$, $\ddot{y}$, $\ddot{z}$; that is to say, we add an acceleration X/m, Y/m, Z/m relative to the frame. We cannot, however, measure this additional acceleration by means of distances travelled relative to the frame without in general bringing in other terms. But we may be able to compare several such imposed accelerations, regarded simply as

relative to the frame, by the method of balancing them against each other; a fact that has an important bearing on Statics. In cases in which we use the earth as the frame, without restriction, we depend on the smallness and constancy of p, q, r.

We have, so far, implicitly assumed that we have a scale for the measurement of time. This is a point of fundamental importance in dynamics, and one which is often dealt with in an unsatisfactory way. The scale is dependent on definition, and as a matter of logic is arbitrary, though the arbitrariness has been to some extent over-ruled by custom. It would be possible to define uniform time by any continuous time measurer, and to adapt the whole of science to this definition, though it might drive us into having to date every experiment in any way involving time in order to make it intelligible. The current conception of uniform time seems practically to amount to the adoption of a method of measurement which shall render this dating unnecessary, except when some material change in the conditions of the experiment is taking place, other than the mere lapse of time. A definition based upon this conception seems to take precedence of any other that can be given, inasmuch as it is certain that, so long as it gives consistent results, any other definition which did not agree with it would have to give way. We may refer to the method of measurement of time given by such a definition as the " repetition " method. It is that on which the construction of all clocks is based. All clocks aim at repeating some physical operation identically, and counting the repetitions. The adoption of the proposed definition would imply the assumption that all clocks of perfect construction agree, whatever be the nature of the operation employed for repetition. The diurnal revolutions of the fixed stars are referred to as a practical standard, found by experience to be superior in accuracy to all clocks, and we may regard this as a particular case of a repetition arrangement; but it is obvious that we do not *define* uniform time by the earth's rotation alone, since we admit speculations as to variations in the rate of this rotation. It is stated in some of the text-books that our definition of uniform time is provided by the law of inertia ; and a reason which may be given for this is that practically the Galileo-Newton theory is appealed to when we question the uniformity of the earth's rotation. But, though we may have faith enough in the theory to choose to make this appeal, it does not follow that the definition ought to be based upon this law. The suggestion in Love's *Theoretical Mechanics* that we should adjust the definition of time so as to save the law of gravitation is one of the same character. It seems better to found it on the whole body of science, if this is possible, rather than to adjust it so as to save any particular

law. To suppose all science to be dynamics would not upset this position; for we should still give a preference to repetition arrangements if a conflict arose. In defining uniform time by the repetition method, we appeal, not to any particular type of experiment, but to the general concurrence of the results given by all physical operations capable of approximate repetition. It may be noted that Newton calls the standard uniform time, which he postulates, " absolute " time; and that he mentions the recently invented pendulum clock as affording one piece of evidence of the need for the adoption of a measure of time superior to that defined by any particular example of motion.

<div style="text-align: right">W. H. MACAULAY.</div>

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REVIEWS.

The Universal Solution for Numerical and Literal Equations, by which the Roots of Equations can be expressed in Terms of their Coefficients. By M. A. M'GINNIS. Pp. x., 196 (Swan, Sonnenschein & Co.), 5/-

It is amusing to find a thoroughly paradoxical work like this, which has escaped the eye of a publisher's reader. It has all the usual features; the laboured proof of the obvious, the misunderstanding of the question at issue, and the insertion, as it were in passing, of a *petitio principii* which ruins the whole argument. It would be easy to make fun of poor Mr. M'Ginnis, with his childish conceit and amazing incompetence; but it will be more useful to say a few words on the problem which he has attacked, because it is so frequently misunderstood, and the facts that are known about it are so often incorrectly stated.

The well-known formula

$$x = \frac{-b \pm \sqrt{b^2 - 4ac}}{2a}$$

for the solution of the quadratic $ax^2 + bx + c = 0$, gives us directions for performing certain arithmetical operations upon the coefficient; besides the four ordinary operations we have to extract a square root. A similar formula can be given for solving a cubic equation: in this case, however, the chain of operations includes a square root, and at least one cube root. These formulae, moreover, apply to the equations, *whatever their coefficients may be*, provided, of course, that they represent ordinary arithmetical or algebraic quantities. Now, what Abel proved is, that a root of a general equation of a degree higher than the fourth cannot be specified by any rule which starts with the coefficients and combines them by a finite chain of operations, restricted to the four rules of arithmetic and the extraction of roots; more briefly, there are no general formulae for equations of the fifth and higher orders which are analogous to those which have been formed for orders lower than five. This has been proved, and no mathematician, acquainted with the

subject, wastes his time upon a problem which has been shown to be impossible. But algebraic formulae can be given for certain special equations of every degree; for instance, the solution of $x^7 - a = 0$ is given by $x = \sqrt[7]{a}$: thus the question arises,—What are the equations which can be solved algebraically, *i.e.* by the four elementary operations and the extraction of roots? This problem was put by Abel, partly answered by him, and practically solved in its general form by Kronecker. There are, however, many interesting points which remain for discussion.

It may be added that formulae for the solution of the *general* equation of the fifth order have been constructed, which involve certain transcendental functions, such as elliptic functions, elliptic modular functions, or the icosahedral function. These rules are analogous to that for the extraction of roots by logarithms, or the solution of a cubic by trigonometrical tables.

It is not superfluous to point out that all this is quite independent of processes for the approximate calculation of the numerical values of the roots of an equation with given numerical coefficients. Except for the labour involved, this can always be done to any prescribed degree of accuracy for an equation of any order. A great deal of Mr. M'Ginnis's book is quite irrelevant to the problem he professes to solve, and consists of numerical approximations by trial and error, which are often ingenious enough, but quite beside the point.

It may be worth while to point out the weak point in Mr. M'Ginnis's "General Solution of the Sixth Degree." He assumes

$$x^6 + mx^5 + nx^4 + ox^3 + px^2 + tx + q$$
$$= \left(x^2 + \frac{m}{a}x + y\right)\left(x^2 + \frac{m}{b}x + z\right)\left(x^2 + \frac{m}{c}x + w\right), \quad \ldots\ldots\ldots\ldots(1)$$

and then puts

$$n - \frac{m^2}{A} = \frac{A_0}{2m} - \frac{m^2}{2A^2} = y + z + w, \quad \ldots\ldots\ldots\ldots\ldots\ldots(2)$$

$$p - \left(\frac{m^2 n}{B^2} - \frac{m^4}{B^3}\right) = \frac{Bt}{m} = yz + zw + wy. \quad \ldots\ldots\ldots\ldots\ldots(3)$$

Thus

$$oA^3 - 2mnA^2 + 2m^3 A - m^3 = 0,$$

$$tB^4 - mpB^3 + m^3 nB - m^5 = 0,$$

whence A, B are to be found: then (2), (3) with $yzw = q$ gives y, z, w by a cubic equation. Finally, Mr. M'Ginnis says "it is evident" that, by comparing coefficients in (1), $\frac{1}{a}, \frac{1}{b}, \frac{1}{c}$ can be obtained. Quite apart from this, there is the fatal flaw that we have 8 unknowns, y, z, w, a, b, c, A, B, with 9 equations to be satisfied: namely, the four included in (2), (3), and the five obtained by equating coefficients in (1). Thus there is a necessary condition to be satisfied by the equation, and I leave it to others to express this in its simplest form.

Mr. W. M. H. Woodward professes to demolish the proof of the impossibility of solving the general quintic by radicals given (after Wantzel) in Serret's *Algèbre Supérieure*. He entirely fails to appreciate

the argument, and asserts that "The conclusion of Wantzel, that the roots cannot be indicated in algebraic language, is equivalent to saying that there are no roots"!

At the beginning of his preface Mr. M'Ginnis tells us that his book appears "at the request of many able mathematicians, teachers, and scholars throughout the United States," and follows this by a list of "a few" of them, which includes, with others, two professors of mathematics (one of whom professes "Languages" as well), a President of a College, a Principal of a High School, and a State Superintendent. Assuming that they have not been unkind enough to play a practical joke, it is difficult to form a high opinion of their intellectual capacity.

G. B. MATHEWS.

Proportion; a Substitute for the Fifth Book of Euclid. By Professor G. A. GIBSON, M.A., F.R.S.E. (John Lindsay, Edinburgh; 8vo., pp. 27.)

Prof. Gibson's pamphlet has received the formal approval of the Edinburgh Mathematical Society, and is printed in the *Proceedings* of the Society for the current year. We welcome it as a genuine and not unsuccessful attempt to provide a satisfactory substitute for the Fifth Book of Euclid. By a fortunate coincidence, it appears at the same time as a more elaborate attempt of the same nature by Prof. M. J. M. Hill, F.R.S. ("Euclid, Books V. and VI.," Cambridge University Press.) The coincidence will have happy results if it leads to a general discussion and to some practical improvements in the teaching of proportion in elementary geometry. That such improvements are much needed is very clearly shown by Prof. Hill, both in the book referred to and in the *School World* for September and October, 1899. We hope that Prof. Hill's work will be reviewed later in these columns, and we refer to it at present solely for the sake of comparison.

In criticising Prof. Gibson's pamphlet we are not questioning its present opportuneness and value, but merely giving expression to personal views and predilections on a debateable question of method and procedure. Prof. Gibson advocates an entire departure from Euclid's method by recommending two fundamental alterations: first, that ratio should be defined as a number from the outset, and second, that the consideration of the ratio of like commensurable magnitudes should be separated from and precede the consideration of the ratio of like incommensurable magnitudes. In § 3 it is apparently implied, though not formally stated, that the symbol $\frac{m}{n} A$ represents m times the nth part of the magnitude A, when m and n are positive integers. In § 6 the following definition is given:—"If A and B be two like magnitudes having a common measure M, so that $A = m M, B = n M$, and therefore $A = \frac{m}{n} B$, the ratio of A to B is defined to be the fraction $\frac{m}{n}$." In § 16 the definition is extended to incommensurable magnitudes as follows:—"If A, B are two like incommensurable magnitudes, and if B be divided into any number n of equal parts of which A contains

more than m but less than $m+1$, so that $A > \dfrac{m}{n} B$ but $A < \dfrac{m+1}{n} B$, then the ratio of A to B is defined to be the irrational number which is greater than every number of the set $\dfrac{m}{n}$ and less than every number of the set $\dfrac{m+1}{n}$." There is a simplicity and likeness in these definitions which is certainly attractive; but it may be questioned whether the second definition is quite satisfactory. Prof. Gibson proves that there is not more than one irrational number which can satisfy the definition, but he does not prove that there actually is one number which satisfies it. It is the existence of one and only one such number which corresponds to what Prof. Hill calls the fundamental proposition in the theory of ratio;[1] although he considers the proof too difficult to find a place in an elementary text-book. The existence of the number, if not proved, should be explicitly stated as an assumption or given as an axiom.

There are other parts of Prof. Gibson's exposition, such as §§ 11, 12, which do not appear to be sufficiently clear. But these, if we understand them aright, are but minor defects which can be easily rectified.

On one point there is universal agreement, that Euclid's Fifth Book, notwithstanding its admirable treatment of proportion, is too difficult for even the most intelligent students. There is also a general agreement that the usual way of meeting the difficulty, which consists in employing two methods that are not equivalent, viz. starting with the definitions of the Fifth Book and then giving algebraic demonstrations of the properties of ratio, is not only indefensible, but the cause of much confusion. Unfortunately there is not agreement as to the best way of meeting or remedying the difficulty. It is to be feared that an agreement on this point will not be arrived at until Universities, Colleges, and Schools are brought into closer touch with one another. What seems to be needed is the formation of an authoritative mathematical board, representing both advanced and elementary education, whose recommendations in respect to teaching and examination would be generally adopted. Little or no improvement and advance in mathematical teaching can be expected unless Cambridge can be induced to exert its proper influence.

Prof. Gibson thinks that the best way out of the difficulty is to give up any attempt to discover a distinctively geometrical method of treating proportion, and to fall back on the results of higher arithmetic and algebra. Prof. Hill's aim is to simplify Euclid's method by discarding the unessential and most difficult parts, and to make it more geometrical, or rather graphical. By whatever method the subject may be approached certain inherent difficulties are encountered which cannot be avoided. In the geometric method the idea of ratio as a relation existing between any two like magnitudes is gradually developed, and only after the idea has become fully established and taken definite shape is it shown that ratio is measurable by a number. We think that Prof. Hill has successfully shown that the geometric

[1] *Cambridge Philosophical Transactions*, vol. xvi., p. 244.

method is not intrinsically more difficult than the arithmetic, and that it can be presented in a less abstract and consequently more easily intelligible way to the learner. If this is so, there are some strong reasons why the geometric method should be preferably adopted, one being that it can be learned at an earlier stage, and another that the two methods are independent, each serving to illustrate the other, and forming when combined a more complete and convincing whole than would result from the arithmetic method alone.

F. S. Macaulay.

MATHEMATICAL NOTES.

84. [**K. 13. a.**] In connection with Problem 372 (Vol. I., no. xxii., p. 371), it may be useful to remark that the point (O) common to the planes bisecting AA', BB', CC', ... is not a point which remains fixed in the displacement. But if O' be the (optical) image of O in the plane ABC..., then O' is displaced to the position formerly occupied by O. This point seems to have been noticed first by Prof. Crofton (*Proc. Lond. Math. Soc.*, Vol. IV.), who called attention to the fact that apparently we could construct a centre of rotation in this way, just as is done in a two-dimensional displacement.

It may be worth while to call attention to the points which do remain fixed in a general rigid-body displacement. Since this is a special case of a one-to-one transformation in space, there will be four such points; and these are all at infinity, two of them being the circular-points in a plane perpendicular to L, and the other two coinciding with the point at infinity on L. Here L is used to represent the central axis of the displacement; or a line such that the displacement of the body can be made up of a translation along L, together with a rotation about L.   T. J. I'a. Bromwich.

85. [**K. 20. d.**] *Geometrical proof for* $\cos\dfrac{A}{2}$, $\sin\dfrac{A}{2}$.

Let A be centre of circle of unit radius
Let B be middle point of EC.
From E, B draw perpendiculars EK, BL to AC.

$$\sin\frac{A}{2}=\frac{BC}{1}=\sqrt{CL\,.\,CA}\quad(B=90°)$$
$$=\sqrt{CL}=\sqrt{\tfrac{1}{2}(1-AK)}$$
$$=\sqrt{\tfrac{1}{2}(1-\cos A)}.$$
$$\cos\frac{A}{2}=AB=\sqrt{AL\,.\,AC}=\sqrt{AL}$$
$$=\sqrt{\tfrac{1}{2}(AC+AK)}$$
$$=\sqrt{\tfrac{1}{2}(1+\cos A)}.$$

J. H. Hooker.

86. [**K. 20. d.**] *Proof of the formula* $\dfrac{b+c}{b-c}=\dfrac{\tan\frac{1}{2}(B+C)}{\tan\frac{1}{2}(B-C)}$.

Make $AD=AE=AB$, and draw DM parallel to EB meeting CB in M. Then DM is perpendicular to DB, and

$$\angle ABD=\angle ADB=\tfrac{1}{2}(B+C),\ \angle DBC=\tfrac{1}{2}(B-C).$$

Therefore $\dfrac{b-c}{b+c}=\dfrac{CD}{CE}=\dfrac{DM}{BE}=\dfrac{DM}{DB}\div\dfrac{BE}{DB}=\dfrac{\tan\frac{1}{2}(B-C)}{\tan\frac{1}{2}(B+C)}.$

W. J. Johnston.

87. [**K. 20. d.**] *To show that* $\cos A + \cos B + \cos C \not> \frac{3}{2} \not< 1$ *when* A, B, C *are the angles of a triangle.*

We have

$$\frac{b^2+c^2-a^2}{2bc} + \frac{c^2+a^2-b^2}{2ca} + \frac{a^2+b^2-c^2}{2ab} - \frac{3}{2} \equiv \frac{(b+c-a)(c+a-b)(a+b-c)-abc}{2abc}.$$

∴ provided a, β, γ are all positive we have only to prove the inequality

$$a\beta\gamma - \frac{\beta+\gamma}{2} \cdot \frac{\gamma+a}{2} \cdot \frac{a+\beta}{2} \leqslant 0,$$

or $$\beta(a^2+\gamma^2) + \gamma(a^2+\beta^2) + a(\beta^2+\gamma^2) \geqslant 6a\beta\gamma,$$

or $$a(\beta-\gamma)^2 + \beta(\gamma-a)^2 + \gamma(a-\beta)^2 \geqslant 0,$$

whence the first part follows.

Again, to show $$\frac{b^2+c^2-a^2}{2bc} + \dots + \not< 1,$$

or $$a(b^2+c^2-a^2) + \dots - 2abc \not< 0,$$

or $$(b+c-a)(c+a-b)(a+b-c) \not< 0.$$

A. C. L. WILKINSON.

PROBLEMS.

[Much time and trouble will be saved the Editor if (even tentative) solutions are sent with problems by Proposers.]

380. [**L₁. 11. c.**] (1) A chord of a rectangular hyperbola subtends a right angle at a fixed point. Find geometrically the envelope of the chord.
(2) A and B move on axes Ox, Oy so that the perimeter of the triangle OAB remains constant. Find the envelope of the circle OAB. E. N. BARISIEN.

381. [**J. 1. b.**] If n sets of lawn tennis are played simultaneously by $4n$ persons, and when the sets are finished n more sets are played, find the number of ways in which the second round may be arranged so that in no case the same four persons play together as in the first round. J. R. BURTON.

382. [**D. 2. d.**] Prove that $(\Sigma p_n)^2 + (\Sigma p_n + 1)^2 = (p_{2n} + q_{2n})^2 = q^2_{2n+1}$, where Σp_n signifies the sum of the even convergents of $p^2 - 2q^2 = \pm 1$. [*Ex.* $n=3$. $119^2 + 120^2 = (99+70)^2 = 169^2$.] R. W. D. CHRISTIE.

383. [**D. 2. d.**] Prove that

$$\left(a + \frac{1}{b} + \frac{1}{c} + \frac{1}{a} + \dots\right)\left(b + \frac{1}{c} + \frac{1}{a} + \frac{1}{b} + \dots\right)\left(c + \frac{1}{a} + \frac{1}{b} + \frac{1}{c} + \dots\right) = \left(t + \frac{1}{t} + \dots\right),$$

where $$t = \Sigma a + abc.$$ R. W. GENESE.

384. L₁. **17. e.**] Two conics intersect at right angles at each vertex of a given right-angled triangle. Shew that they must be confocals, or if not, find the locus of their remaining point of intersection. E. INNES.

385. [**K. 13. a.**] Shew that any displacement of a rigid body is equivalent to a series of reflexions in plane mirrors of the points of the body.
W. J. JOHNSTON.

386. [**L₁. 3. c.**] If a circle through the centre of an ellipse cut pairs of conjugate diameters in A, A'; B, B'; ... then shall the chords AA', BB', ... all pass through a fixed point. A. LODGE.

387. [**K. 17. e. 20. f.**] Construct a spherical quadrilateral, given a, β, γ, δ, the mid points of the sides taken in order; and prove that the cosine of half its spherical excess is equal to

$$\cos \beta\gamma . \cos a\delta - \cos \gamma a . \cos \beta\delta + \cos a\beta . \cos \gamma\delta.$$

C. E. M'Vicker.

388. [**K. 11. e.**] The line $ABCD$ is cut harmonically in B and C; on AC and BD as diameters circles APC, BQD are described whose planes intersect at right angles, and PQ, PB, QC are drawn. Shew that

$$PQ . BC = PB . QC.$$

A. S. Toller.

389. [**J. 2. c.**] A straight line is broken at random into any number of parts, and on every part a square is described. Shew that, if the number of parts does not exceed *ten*, the expectation of the largest square will be greater than the expectation of the sum of all the other squares.

W. A. Whitworth.

SOLUTIONS.

Unsolved Questions.—171, 275, 279, 283, 285, 326-7, 336-8, 341, 349, 356, 369, 370, 372-3, 376-9.

The question need not be re-written; the number should precede the solution. Figures should be very carefully drawn **to a small scale** on a separate sheet. Solutions will be published as space is available.

**57.** [**X.**] *Draw graphs illustrating the solution of the equations*

$$x^2 = ax + by; \quad y^2 = bx + ay$$

for different values of a and b.

E. M. Langley.

Solution by W. E. Hartley.

Since neither of the equations is altered by changing the signs of all the four quantities a, b, x, y, we may confine our attention to the case in which b is positive: the two equations represent (i) the parabola $x^2 = ax + by$ (of latus rectum b) passing through the origin, having its vertex at $\left(\dfrac{a}{2}, -\dfrac{a^2}{4b}\right)$ and its axis parallel to the positive direction of Oy, and (ii) the image of (i) in the line $x = y$. We have therefore to consider the intersections of the parabola $x^2 = by$ with its image in a chord parallel to $x = y$; two (of the four) intersections are always real, viz. the intersections of the line and the parabola; the other two are imaginary or real according as the line does or
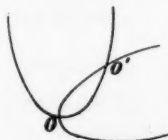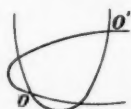
does not pass between the parallel normal and the vertex, *i.e.* according as a does or does not lie between the limits b and $-3b$. Two figures are drawn to illustrate the two cases, the first corresponding to values $\sqrt{10}$ and $-(2+\sqrt{10})$ of the ratio $\dfrac{a}{b}$ according as O or O' is the origin, and the second to values $\dfrac{1}{\sqrt{2}}$ and $-\left(2 + \dfrac{1}{\sqrt{2}}\right)$.

129. [**K. 11. e.**] *There is a half circle as ABC, whose diameter is AB, upon which is made another lesser half circle whose diameter is AE, so that the greater half circle being divided into two equal parts with the line DC, doth pass through the two circles in the points F, C, so that the part CF is 6 and the part BE is 9. How much is the diameter of each of these two circles?*

(Captain Rudd's *Practical Geometry*, London, 1650).

SOLUTION BY W. E. HARTLEY, AND C. E. YOUNGMAN.

We have
$$\frac{DF}{DE} = \frac{AD}{DF} = \frac{AD-DF}{DF-DE} = \frac{CF}{BE-CF} = \frac{6}{3},$$
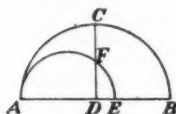
and $DF - DE = 3 = 6 - 3$;

$\therefore \quad DF = 6, \quad CD = 12, \quad AB = 24, \quad AE = 15.$

Otherwise $DB^2 = DE \cdot DB + DB \cdot EB = FD^2 + DB \cdot EB$;

$2CF \cdot CD + FD^2 = CF^2 + CD^2,$

$\therefore \quad 2CF \cdot CD = DB \cdot EB + CF^2,$

$\therefore \quad CD$ or $DB = CF^2/(2CF - BE) = 12,$ etc.

144. [**I. 1.**] *Supposing packs of cards to be arranged in all the possible 52! ways, each pack taking up one cubic inch of space, calculate approximately or exactly the size of a cubical box which would hold all the lot; and the length of time a ray of light would take to travel from one corner to the farthest opposite.* R. P. ROYSTON.

Solution by W. E. HARTLEY.

$52! = 2^{49} \cdot 3^{23} \cdot 5^{12} \cdot 7^8 \cdot 11^4 \cdot 13^4 \cdot 17^3 \cdot 19^2 \cdot 23^2 \cdot 29 \cdot 31 \cdot 37 \cdot 41 \cdot 43 \cdot 47$;

extracting the cube root and dividing by $63.360 = 2^7 \cdot 3^2 \cdot 5 \cdot 11$, we get for the side of the box in miles

$2^9 3^5 5^3 7^2 \cdot 13 \cdot 17 \times \sqrt[3]{(2 \cdot 3^2 \cdot 7^2 \cdot 11 \cdot 13 \cdot 19^2 \cdot 23^2 \cdot 29 \cdot 31 \cdot 37 \cdot 41 \cdot 43 \cdot 47)}$.

Using logarithms we find for this $10^{17} \times 6.81923$.

[Otherwise

$2^9 \cdot 3^5$, etc. $= 10^3 \times 64 \times 81 \times 49 \times 51 \times 13 = 10^3 \times 64 \times 1053 \times 2499$

$= 10^3 \times 168412608.$

$2 \cdot 3^2 \cdot 7^2$, etc. $= 98 \cdot 99 \cdot 13 \cdot 437^2 \cdot 899 \cdot 1517 \cdot 2021$

$= 437 \times 43263 \times 88102 \times 1517 \times 26273$

$= 23 \times 19 \times 3811557826 \times 39857141$

$= 10^{15} \times 87.66583 \times 757.285679 \left(\text{to within } \frac{1}{4 \times 10^{10}} \text{ of whole} \right),$

and cube root $= 10^5 \times 4.4423 \times 9.1149 \quad \left(\dots\dots\dots \frac{1}{5 \times 10^4} \dots\dots\dots \right)$

$= 10^5 \times 40.4911 \quad (\dots\dots\dots\dots\dots\dots\dots),$

$\therefore$ side of box in miles $= 10^{17} \times 1.6841 \times 4.0491 \quad \left(\dots\dots\dots \frac{1}{3 \times 10^4} \dots\dots\dots \right)$

$= 10^{17} \times 6.8191 \quad (\dots\dots\dots\dots\dots\dots\dots),$

so that we may take $10^{17} \times 6.819$ as correct to last figure.]

This gives for the diagonal $10^{18} \times 1.18113$ miles, and taking velocity of light as 186,000 miles per second $= 5.86959 \times 10^{12}$ miles per year, we get for the passage of light along diagonal a period $10^5 \times 2.01228$ years $= 201,228$ years.

152. [**I. 1. 24. a.**] *If* $z = \cdot9999999999$ *and* $e = 2\cdot71828$, *find the value of* $z + \frac{1}{2}z^2 + \frac{1}{3}z^3 \ldots$ *to four decimals.*

<div align="center">Solution by W. E. HARTLEY.</div>

$$z + \tfrac{1}{2}z^2 + \tfrac{1}{3}z^3 + \ldots = -\log(1-z) = \log 10^{10} = 10[\log 8 + \log \tfrac{5}{4}]$$

$$= 10\left[3\log\frac{1+\frac{1}{3}}{1+\frac{1}{3}} + \log\frac{1+\frac{1}{9}}{1-\frac{1}{9}}\right].$$

$\dfrac{1}{3} = \cdot33333333+$	$\dfrac{1}{3^3} = \cdot03703704$	$= 20\,[1\cdot039721$ $\qquad\quad + \cdot111572]$
$\dfrac{1}{3\cdot3^3} = \quad 1234568 -$	$\dfrac{1}{3^5} = \quad 411523$	$= 23\cdot0259$
$\dfrac{1}{5\cdot3^5} = \quad 82305 -$	$\dfrac{1}{3^7} = \quad 45725$	
$\dfrac{1}{7\cdot3^7} = \quad 6532 +$	$\dfrac{1}{3^9} = \quad 5081$	
$\dfrac{1}{9\cdot3^9} = \quad 565 -$	$\dfrac{1}{3^{11}} = \quad 565$	
$\dfrac{1}{11\cdot3^{11}} = \quad 51 +$	$\dfrac{1}{3^{13}} = \quad 63$	
$\dfrac{1}{13\cdot3^{13}} = \quad 5 -$		

$\therefore \quad \frac{1}{2}\log 2 = \cdot346,57359(\pm 2)$,

the residue of series being less than $\dfrac{1}{17\cdot3^{17}(1 - \frac{1}{9})}$,

<div align="center">i.e. $< \cdot0000000005.$</div>

$$\frac{1}{9} = \cdot11111111 + \frac{1}{9^3} = \cdot00137174,$$

$$\frac{1}{3\cdot9^3} = \quad 45725 - \frac{1}{9^5} = \quad 1693,$$

$$\frac{1}{5\cdot9^5} = \quad 339 - \frac{1}{9^7} = \quad 21,$$

$$\frac{1}{7\cdot9^7} = \quad 3 -$$

$\therefore \quad \frac{1}{2}\log \frac{5}{4} = \cdot11157178(\pm 1)$,

and residue $< \dfrac{1}{98\cdot80} = \cdot0000000003.$

<div align="center">[v.] ON MARKING EUCLID PAPERS.</div>

A correspondent laments the absence of general agreement as to the deduction of marks for mistakes in Examination answers. He suggests, as an experiment, that readers of the *Gazette* should state on a post-card addressed to the Editor, the deductions they would make in the following cases. A résumé of the results will appear in a subsequent number of the *Gazette.*

(1) Euc. i. 37. (Full marks 100.)

In shewing that the triangle is half the parallelogram, what should be deducted for omission of the words "for the diagonals bisect the parallelogram" (*a*) without reference, (*b*) quoting as a reason the enunciation of I. 41 ?

(2) Euc. iv. 4. (Full marks 100.)

What deductions for (a) commencing by saying "Bisect the three angles of the triangle, meeting in O;" (b) in proving the triangles equal, omitting to point out that the common side is opposite the equal angles in the two triangles?

BOOKS, MAGAZINES, ETC., RECEIVED.

Two Geometrical Transformations. By J. A. THIRD, M.A., D.Sc. (From Proc. Edin. Math. Soc. XVIII. 1899-1900. pp. 14.)

†*Proportion*: *A Substitute for Euc. V.* By G. A. GIBSON, M.A., F.R.S.E. pp. 27. 1900. Lindsay (Edinburgh.)

Supplemento al Periodico di Matematica. April, June, 1900. Fasc. VI.-VIII. Edited by Prof. LAZZERI. (Livorno.)

*L'Elimination. By H. LAURENT. (No. 7 Scientia.) Carré & Naud. 1900. (Paris.)

*Geometrical Drawing. By W. H. BLYTHE, M.A. Part I., Plane and Elementary Solid; Part II., Solid or Descriptive Geometry; pp. 328. 1900. (Cam. Univ. Press.)

An Essay on the Foundations of Geometry. By B. A. W. RUSSELL, M.A. 7s. 6d. pp. 201. 1897. (Cam. Univ. Press.)

*The Fifth and Sixth Books of Euclid, arranged and explained. By M. J. M. HILL, F.R.S. pp. 143. 1900. (Cam. Univ. Press.)

Official Year-Book of the Scientific and Learned Societies of Great Britain and Ireland. pp. 292. 1900. (C. Griffin & Co.)

†*The Universal Solution for Numerical and Literal Equations.* By M. A. M'GINNIS. pp. 195. 1900. 5s. (Swan Sonnenschein.)

Non-Euclidean Geometry for Teachers. By GEORGE BRUCE HALSTED. (Popular Astronomy.)

*Theory of Differential Equations. By Professor A. R. FORSYTH, F.R.S. Part II. (Vols. II. and III.) 20s. net. 1900. (Cam. Univ. Press.)

The Solar Eclipse of May 28th. By G. H. BRYAN, F.R.S. (University Correspondent. June 14, 1900.)

The Principles of Chess in Theory and Practice. By JAMES MASON. Third Edition, revised and enlarged. pp. 327. (2s. 6d. net.) (Horace Cox.)

Review of Hertz's Principles of Mechanics. By Pat. DOYLE, C.E. (Indian Engineering, July 7, 1900.)

*Leçons sur la Théorie des Formes et la Géométrie Analytique Supérieure. By H. ANDOYER. Tome I. pp. vi.-508. 1900. 15 fr. (Gauthier-Villars.)

Journal de Mathématiques Elémentaires. Prof. MARIAUD. June and July, 1900. (Delagrave.)

*Exercises in Graphic Statics. By G. F. CHARNOCK. Part I. 40 sheets. 1900. (J. Halden & Co.)

On an Extension of the Wallace Theorem. By Dr. N. QUINT. Reprinted from *Nieuw Arch. voor Wisk.*

Sulla Definizione del Numero. By Prof. R. BETTAZZI. (Per. di Mat.) Reprint.

*A Brief History of Mathematics. Translated from Dr. KARL FINK's *Geschichte* by Prof. W. W. BEMAN and D. E. SMITH. pp. 333. 6s. net. 1900. (Kegan Paul.)

*The Student's Dynamics. By Professor G. M. MINCHIN, F.R.S. pp. 255. 3s. 6d. (Bell.)

The Proceedings of the Edinburgh Mathematical Society. Vol. XVIII. 7s. 6d. 1899-1900. pp. 26, 104. (Williams & Norgate.)

* Will be reviewed shortly.   † Reviewed in this number.